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Theo A. F. Kuipers

VERSTEHEN, EINFÜHLEN AND MENTAL SIMULATION

REPLY TO ANNE RUTH MACKOR

Anne Ruth Mackor introduces some very intriguing questions of methodology in the social sciences by demanding attention to the simulation theory. This theory is supposed to be an alternative to the “theory theory” about the mind, as far as our ability “to ascribe mental states to other persons and to ourselves” is concerned. Interestingly enough, the simulation theory has similarities with the old idea of “verstehen” or rather “einfühlen” as a prerequisite for doing (folk-psychology-based) social science. As Mackor reports, I have claimed in the latter connection in SiS that it is not necessary for us to assume that to explain Hitler’s behavior we have to be a bit like him.

In this reply I shall first try to summarize the main claims that have been made. I then will discuss the reach of the experimental evidence which Mackor presents, and I suggest an additional perspective.

Who Claims What

To begin with, I am not so sure as Mackor is that Van Nierop is going further than claiming that “verstehen” of certain beliefs and desires is a general prerequisite for doing social science. He only claims that, for Dilthey at least, it is a transcendental condition for its very possibility, without having to play a crucial methodological role. Van Nierop (1989, p. 20) writes: “None of the three main moments which he [Dilthey] distinguishes in the interpretation process and develops in their mutual relationship: *Erlebnis* [*Experience*], *Ausdruck* [*Expression*] and *Verstehen* [*Understanding*] are genuine methodological principles. They rather form a framework of conditions for the possibility of interpretation.”¹

¹ “De drie hoofdmomenten die hij in het interpretatieproces onderscheidt en in hun onderling verband ontwikkelt: *Erlebnis*, *Ausdruck* en *Verstehen* zijn geen van drieën echte methodologische

Even in Van Nierop's specific example of understanding war (pp. 51-2), as summarized in Section 2 by Mackor, the points 1-3 about "despair" and "strong desire" precisely suggest not so much the requirement of having the same sensations when understanding war, but being able to imagine having them, that is, the requirement of knowing at all what it is to have such sensations. Here the occurrence of 'never' under '2' seems crucial to me. It leads to the claim of Mackor herself about Van Nierop and, indirectly, Dilthey that, according to them, we ourselves need to have *ever* experienced despair and strong desires. However, according to Mackor, these points have nevertheless some methodological implications for (folk-psychology-based) social scientists.

However, I have to concede, see Mackor's Note 7, that I certainly have been too hasty in suggesting that 'verstehen' and 'einfühlen' are on a par. I even agree that Dilthey and Van Nierop explicitly want to discard 'einfühlen', but I would like to argue that they do not succeed very well in this. Crucial terms like 'despair' and '(strong) desire' not only have cognitive but also emotive connotations. Hence, we seem to be entitled to replace the formulation of Van Nierop and Dilthey's requirement above, viz. "knowing at all what it is to have such sensations," by the requirement of "knowing at all what it is to have such cognitive, that is, verstehende, and emotive, that is, einfühlende, sensations." In other words, despite the fact that Dilthey and Van Nierop explicitly discard the emotive side, by using phrases like 'having experienced despair and strong desires', they in fact suggest that the two aspects, verstehen and einfühlen, are both relevant. Be this as it may, in view of Mackor's exposition of the simulation theory, it is clear that she will agree that the emotive aspect is at least as relevant as the cognitive aspect in understanding human behavior in folk psychological terms.

In terms of the modern simulation theory, as opposed to the theory theory, "mental simulation," that is, "imaginative identification or empathy" is a crucial ingredient in each ascription of a mental state. More specifically, on Alvin Goldman's view, "I must imagine *myself* in the situation of the other." Mackor herself does not go that far. Her methodological claim amounts to: "The argument is not, however, that we must have had the same attitude with respect to the same content, but we must have had the same attitude toward some content, and we must have had some attitude toward the same content" (see the beginning of Section 6). However, the first condition is not case-specific, and hence not methodological. The second condition, viz. "we must have had some attitude toward the same content," is case-specific, but *prima facie* rather vague. I

principes. Ze vormen veeleer een stramen van voorwaarden voor de mogelijkheid van het interpreteren."

certainly have “some attitude” to wars in general and to the Second World War and Hitler in particular. I shall discuss Mackor’s more specific claim in this respect in some more detail below.

The Reach of Experimental Evidence

In Section 4 Mackor presents a number of experimental results as part of an exploration of the paper’s leading question “what role simulation plays in folk psychology-based social science?” Her ultimate concern goes even further: “to what extent [might] the simulation theory [] cause trouble for our naturalist view of the *relation* between the natural and social sciences?” The leading question of the paper can be split into at least three questions. One, what role does simulation play in folk psychology? Sections 3 and 4 are in fact restricted to this question. Two, what role does simulation play *as a matter of fact* in folk-psychology-based social science? Three, what role *could and should* simulation play in folk-psychology-based social science? Sections 5-7 certainly deal with the third question and to some extent with the second.

In Section 4 Mackor reports a number of experiments that seem to be relevant to the debate between the theory theory (TT) and the simulation theory (ST) about the nature of folk psychology. Regarding the false-belief task, the most detailed example in Section 4, Mackor herself concludes that it “offers no conclusive evidence in favor of either TT or ST.” According to Mackor the second experiment about failing to catch the plane seems to be in favor of ST. Recall that no fewer than 96% of the investigated subjects expect that Mr Crane, who arrives 30 minutes too late to catch a plane that departed according to schedule, will be less upset than Mr Tees, who arrives 5 minutes too late to catch another plane that happened to be delayed for 25 minutes. Mackor posits that according to TT the subjects predict on the basis of a statistical guess “*most persons* are [or will be] more upset when their plane has just left [, than ...], “which they may even explain and predict in terms of the more general statement that most persons are more upset when failing to achieve some purpose but nearly succeeding as opposed to failing without a real chance of succeeding. However, according to ST people predict it on the basis of the “first-person perspective”: “I myself would be more upset if I were in the position of Mr Tees.” According to Mackor “it looks as if simulation theory has got a point here, for will not most persons base their inference, at least in this example, on prior self-knowledge?”

In my view this is a too hasty, albeit tentative, conclusion, for, as in the false-belief task, the evidence does not discriminate between the two theories. It seems plausible that in such cases some substantial percentage of the

subjects responds according to ST (ST-subjects) and the remaining percentage, also substantial, according to TT (TT-subjects). Almost all members of both groups may guess, on their respective grounds, that Mr Tees will be more upset than Mr Crane, hence the experiment does not discriminate. It is likely that the percentages may vary with the kind of case, for several reasons. One reason may be, as Mackor suggests, that in case people have strong feelings about what they would do themselves in the given situation, they are more likely to behave as ST-subjects. However, the more statistical evidence is publicly known about some type of case, the more people will behave as TT-subjects. Similarly, we may expect that scientifically educated people tend more to TT-behavior than other people. Consider the paradigmatic question in The Netherlands or any other country that was occupied in 1939-1945: "What do you think that you would have done under the German occupation in the Second World War: join the resistance movement or join the collaborating party or remain passive?" One may expect that statistically well-informed people think on average that they would have been less brave than those who are not well-informed, despite the fact that in both groups relatively many people have the inclination to think *prima facie* that they would join the resistance.

In sum, the airplane experiment, like the false-belief experiment, is not very helpful for the first question, let alone for the second and the third. Let us now turn to the third question, more particularly the question of whether simulation has to play a methodological, that is, case-specific role in the correct ascription of mental states to others. As suggested, Mackor addresses the second and, even more clearly, the third question in Sections 5-7. Assuming that the first question should be answered positively, that is, assuming that the simulation theory about folk psychology is largely correct, Mackor specifically claims, in regard to the third question, at the beginning of Section 6: "... a social scientist must have a sufficiently rich sensory, cognitive, volitional and affective repertoire, and this repertoire must be sufficiently like the persons he or she studies. ... Moreover, this repertoire must be used (time and again) in the attribution of mental states to other persons."

In Section 6 Mackor presents a detailed analysis of first- and third-person conceptions of mental states. In Subsection 6.5 she arrives under (2) at an underpinning of the latter, methodological, claim: "The neurological and physiological evidence mentioned in the last paragraphs of Section 4 (the discovery of mirror neurons and the physiological experiments of Levenson and Ruef and of Althaus) speak in favor of this view." Although this evidence seems to support the simulation theory as the better theory about the nature of folk psychology, I do not see why this should support the methodological claim about how "folk-psychology-based social science" has to proceed. For

example, we should leave room for other possibilities, not discussed by Mackor in the present paper, but in another publication (Mackor 1997) and elaborated by myself in Ch. 6 of SiS. The case concerns the different explanations of persistent and adolescent delinquent behavior. Let me quote (SiS, p. 185) part of the summary:

In short, persistent delinquent behavior is explained by referring to abnormal biophysical conditions leading to abnormal functional development, which under normal social conditions may lead to persistent delinquent behavior ... Such an abnormal biophysical condition does not play a role in the other type of delinquent behavior. Adolescence delinquents have a perfectly normal functional development, including the (functional) tendency to choose age-specific role models. However, in the absence of classical role models and the presence of other delinquents, of a persistent or adolescence nature, they join delinquent behavior, up to the age that other role models, specific for that age, become dominant. In sum, in the case of adolescence delinquents the external social factors are crucial; they provide the abnormal factors for the specific causal explanation of the delinquent behavior.

It seems clear that the second, role model, explanation can be phrased in folk psychological terms. But for that purpose, do we ourselves need to have experience with criminal role models? It is possible that Hitler falls in a similar category. However, it is also possible that a biophysical explanation has to be given in terms of some kind of brain defect. As has become clear from recent experimental studies (see e.g. Damasio 1994), there are people who have some frontal lobe defect, at birth or later incurred by accident, which seems to be the cause of their having (almost) no emotions at all. In this case the point of a folk-psychology-based explanation is not whether we can simulate Hitler's mental condition, but whether we can imagine what would or could happen if we did not have the kind of emotions we normally have. Hence, in this case too we need not be a bit like Hitler in order to understand his behavior. In sum, folk-psychology-based social scientists not only will have to leave room for both possibilities, they can leave room for them. However, I can perfectly imagine that Mackor will be of the opinion that I am stretching the idea of folk-psychology-based social science much too far.

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